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A N E S S A Y

ON THE

PRINCIPAL CAUSES WHICH UNITE IN PRODUCING
AND DIFFUSING DISEASE;

BEING

THE ADDRESS IN MEDICINE,

READ BEFORE

THE SEVENTEENTH ANNUAL GENERAL MEETING

OF THE

Provincial Medical and Surgical Association,

HELD AT WORCESTER, AUGUST 1ST AND 2ND, 1849.

BY

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P R E F A C E .

THE Author, having been honored with a request from the Council of the "Provincial Medical and Surgical Association," to deliver the general Address in Medicine, at the annual meeting of the Society, in August, conceived that he could not select a subject of more general interest than the consideration of the causes by which diseases are produced and propagated.

Convinced that the conflicting opinions entertained by many members of the medical profession respecting the influence of contagion depend more on imperfect definition of the terms employed than on essential difference as to matters of fact, he has attempted, in the following Essay, partially to remedy this defect by applying the known laws of chemical decomposition to the investigation of the natural distinctions between the specific, endemic, and epidemic classes of disease. He is persuaded that careful study of the different modes in which the causes of these several classes of disease operate on the system, singly or in combination, is capable of rendering the contagious or non-contagious character of any given malady matter of demonstration, instead of resting, as at present, solely on opinion or conflicting evidence; he also believes that it will afford an intelligible explanation of the incessant variations observed in the types of disease, and by increasing the accuracy of diagnosis, give greater precision to practice.

These investigations have necessarily involved the consideration of the principles upon which quarantine and other measures for

the protection of the public health ought to rest, and have given occasion for some discussion of the doctrines propounded by the General Board of Health; not certainly with any intention of needlessly finding fault, but from a conviction that they are in some respects founded upon error that must tend partially to defeat the objects which that Board has laboured so assiduously to promote.

The small degree of influence exerted by medical opinion on questions of this nature proves how much of the public confidence has been lost to the profession by a want of unanimity on the important subject of contagion that is highly derogatory to the dignity of medicine as a science. The Author would, therefore, gladly indulge the hope that some of the obscurity of the subject may have been removed by the arguments here offered for believing that contagion can arise from no cause that is not able to reproduce itself in the system by chemical decomposition of the materials of the body.

94, MOSLEY STREET, MANCHESTER,

October 8, 1849.

ADDRESS IN MEDICINE.

SIR AND GENTLEMEN,—When the founders of this Association instituted the annual Address in Medicine, its objects were fixed and definite; they were to collect and analyze the various contributions of the preceding year to medical science, and to present to the members a mass of valuable information, which few of those occupied with the anxieties of practice could afford time to acquire for themselves.

Thus to select only what was really valuable from the mass of works and periodicals, relating to medicine in all its branches, that yearly issue from the press of various countries, was no ordinary task; for to distinguish new matter from old, truth from false theory, originality from plagiarism, required a rare union of industry and high literary attainments with great professional knowledge and practical judgment.

So well, however, was this difficult duty performed, season after season, by the various members by whom it was undertaken, that these Retrospects were highly appreciated by the profession in general, and thus the devotion of a separate periodical publication to this object was suggested. Two such works now perfectly supply this desideratum; and we owe it to the talents of their editors that this part of the business of the annual meetings of the Association has now become unnecessary. An Address in Medicine is now substituted for the Retrospect, and the selection of the subject is left to the author of the paper. This choice, however, is by no means easy; for as the Retrospect embraced the whole

range of medical and collateral science, and was therefore addressed to no single branch of the profession, an Essay to supply its place must not confine itself to questions of mere practice, nor to a fractional portion of the history of disease, but, like that of last year, involve the discussion of some great physiological law, if it is to be worthy the consideration of all who make the study of life their business or their pleasure.

Such reflections have rendered this undertaking more arduous than it at first appeared; and permit me to say, that anxious as the moment must ever be to one about to address the assembled learning and experience of England on subjects of which every one present is a master, this occasion is to me of even more than usual solemnity; for the place in which accident has called me before you for that purpose, is hallowed to me by the memory of the great interpreter of nature's laws, who guided my first steps in the study of medicine.

It was in this immediate neighbourhood that Sir Charles Bell terminated a career of unwearied devotion to the science of his profession, and the recollection that it had fallen to my lot to address you on ground so sacred, has continually suggested to me how little he would have considered what I have now to offer worthy of being submitted to so distinguished an audience. Thus have I been rendered doubly sensible of my own deficiencies.

The subject to which I would request your attention is—

THE CAUSES WHICH UNITE IN PRODUCING AND DIFFUSING DISEASE.

To attempt to review the whole of this extensive subject within the limits of an Essay, would have been inadmissible under other circumstances; but at the present moment I cannot help thinking it almost imperative on the medical profession, as a body, to give the matter their careful consideration.

During a long period the traditionary terrors of plague had so biassed public opinion, that though several commissions had been appointed by Government to inquire whether facts justify the general confidence in quarantine, as a preservative against the introduction of pestilence into the country, these investigations have hitherto all resulted in the renewed assertion of its efficacy, and of the absolute necessity for its maintenance.

Of late, however, the reappearance of cholera in England, notwithstanding such restrictive measures,—the great expense of the institution—the injury thereby inflicted upon commerce, and cruelty on individuals,—have concurred with a long respite from Egyptian plague, and better acquaintance with the diseases of other countries, to prepare the public mind for the reception of other views, and for the announcement contained in the Report recently issued by the Board of Health, that quarantine is not only useless but injurious.

This Report is written with great talent, and the practical recommendations it contains are in general so excellent, that it has been received with enthusiasm by the public press; yet there is scarcely a medical journal or review by which it is not condemned. This is the consequence of its practical conclusions having been based upon theories widely at variance with the received doctrines of medicine, and of facts having been so mixed up in the Report with what are assumed to be general laws of disease, that it is difficult to concur with the conclusions, without giving apparent assent to the speculations on which they are founded.

So decided a difference of opinion as thus results between the general and the medical press, respecting this Report, is calculated to injure the profession in the estimation of the public, by fostering a false notion that, as a body, medical men are opposed to all improvement, where so much is obviously required.

It therefore has become a matter of some importance to show that we are not in reality so ignorant of the means by which diseases are diffused as we are sometimes represented.

A tribunal, constituted like the Board of Health, has but one legitimate mode of arriving at its conclusions, viz.,—the careful examination of direct evidence: but with the medical profession it is different; for it rests with them to study the subject through

the medium of those great general laws and principles according to which (when ascertained) we find the designs of the Creator uniformly carried out. The Commission, however, has not been content to confine its investigations to direct evidence, and has ventured into the depths of theoretical medicine, propounding doctrines which, however novel and popular, are far from being proved, and demand our careful consideration.

This invasion of our peculiar province is a direct challenge to us to meet them upon the principle; and, lest we might incline to shun an engagement, we are further urged by an insinuation that the medical profession is, and always has been, in retard of the laity in practical knowledge of this subject. There remains to us, therefore, no choice, but tacitly to acknowledge the Board of Health as the highest authority on one of the most recondite portions of medical science, or to enter fairly into the discussion, not as mere critics of this Report, but as students of an important branch of theoretical medicine.

In commencing this investigation, it cannot be considered humiliating to confess that very great, and apparently irreconcilable, difference has long existed among medical men, regarding the exact amount of influence exerted by contagion in the propagation of disease, for evidence on this point is always most contradictory; and until lately we can scarcely be said to have been in possession of the elements on which to reason correctly on the subject.

It would be difficult to determine at what period contagion first began to be recognised as a source of disease; but previous to the introduction of small-pox and syphilis into Europe, it was certainly not considered by any writer so omnipotent a cause as it has since been esteemed by many. Even at the present day, notwithstanding the great amount of discussion the subject has undergone, it can scarcely be said that more than two points are to be held as universally acknowledged by the profession. One, that certain diseases, known as specific, are always communicated by contagion or infection, (which terms will presently be shown to mean exactly the same thing;) the other, that intermittent and irritative fevers are incapable of being so communicated.

But if the term specific include only the exanthemata, pertussis, syphilis, hydrophobia, and a few others, and these alone be held

indisputably contagious; and if only the intermittent, traumatic, and hectic fevers be acknowledged by all to be incommunicable, it is evident that the great majority of maladies will remain to be registered as doubtful; and in this doubtful class will be included all the great plagues and pestilences of history, as well as many other less widely-spread diseases.

Of these there is scarcely one that has not given rise to hot discussion of the question, whether each individually is or is not contagious; and as a class, these diseases have become the origin of two nearly equally numerous parties in the profession—the contagionists and anti-contagionists. It is impossible to deny that both parties are perfectly honest in their convictions; yet, that upon this subject the most diametrically opposite conclusions have been drawn by the greatest authorities in medicine, from an equal amount of experience, and after a lifetime spent in the investigation of disease. It is, therefore, a legitimate conclusion that both are partly right and both partially wrong who profess extreme opinions; and the probability is, that some element of the question has been equally overlooked by both, the investigation of which might tend to reconcile their differences.

It will be remembered by those present at the last annual meeting, that the question of contagion was alluded to, or I might rather say commenced, in Dr. Davies's highly philosophical Essay, by an attempt to classify all fevers, as respects their infectious or non-infectious character, according to the nature of their exciting causes.

The principle propounded was, that a poison which is formed in the living blood will find in the blood of others the elements of which it is composed, and therefore may be there reproduced, and thence infinitely propagated. But if the poison be formed extraneously to the body,—from elements which do not exist in the blood,—transmutation of the materials of the blood into this poison is impossible, and not being reproduced in the system, the disease it causes is necessarily non-contagious.

It is evident that this principle must constitute the basis of all argument on the subject, and probably has been long so considered, though it is only recently that the progress of organic chemistry has placed us in a position to reason upon it correctly. But this conclusion may be logical and its principle correct, and yet not be sufficiently definite to apply directly to every cause of fever.

If none were contagious but those which are specific, or if all contagious fevers were the effect of poisons generated only in the living blood, it would meet every contingency ; but each day adds fresh evidence that this is not the case, and that certain poisons formed extraneously to the body are capable of reproducing themselves in the blood, and of communicating disease to others.

To prepare for the enquiry into the nature of such extraneous causes, it will be necessary, first, to define what is essential to those poisons which are never generated except in the living body, viz., specific virus ; for by determining this, we shall be better prepared to distinguish what is essential to the cause of other contagious maladies, and what to that other class of causes whose elements do not exist in the blood, which do not produce communicable disease, and which belong to what Sydenham has designated the general epidemic constitution of the air.

If we review the mass of facts now collected, relating to the origin, course, and modes of propagation of disease, with the aid of that knowledge of the chemical actions which influence the decomposition of organic bodies, inculcated by Liebig, it will be found that all diseases that have been vulgarly called epidemic, from their prevalence at the same time and place, are products of one of three distinct and separate causes, or of two of these causes combined, or of all three operating together on the same individual.

Of these the first is *Specific Poison* ; the second, *Putrefaction* ; and the third, *Epidemic Influence* ; and the remarks which are to follow are intended to prove that the first cause, " Specific Poison," is generated only in the *living* blood, is propagated only by contact, or infection, that it never produces any form of disease but its own, and that it is capable of inducing its own peculiar action, not on the whole essential elements of the blood, but only on a special material combined with them, which is not in general essential to the organism.

We consider the action of this class of morbid poisons to be defined in the following sentence, from Liebig's " Organic Chemistry." He says, " If the exciter" (to chemical action) " be able to impart its own state of transformation to *only one* of the component parts of a mixed liquid, its own reproduction may be the consequence of the decomposition of that one body."

The specific virus is here the exciter, and a particular material

combined with, but not necessarily essential to the blood, is the one component part of the mixed fluid to which the exciter can impart its own condition, and turn it into virus.

The second cause, or "Putrefaction," is that recognised chemical power by which organic particles in a state of active decomposition are enabled to induce the same kind of chemical action in bodies of similar chemical composition to which they are applied; but this cause differs from the first, inasmuch as it is capable of acting on the whole, or nearly the whole, mass of the blood and tissues, and is not limited in its operation to a special material *associated* with them; and, although its action is capable of being induced in the living body, and of being reproduced there, and of being thence communicated to others, it does not, like specific poisons, necessarily originate in the living blood.

The third cause is that which is properly termed "Epidemic Influence," or the *superimposed cause of disease*, which is not like the other two, a product of the decomposition of organic matter,—which finds no material in the body wherein it can produce propagable chemical action,—which cannot, therefore, be reproduced in the blood, or by any possibility become contagious, but which operates like other inorganic poisons, such as the various noxious gases, or vegetable or mineral poisons; or, it may be, like electricity, by affecting the nervous system, and thereby (mediately) disturbing the vital functions.

Respecting the origin of contagious fever, Dr. Mason Good quotes the remark of Sir Gilbert Blane, "That there is not a secretion or exhalation of the human body which may not be so vitiated as to produce diseases, communicable to others by contact or respiration, under fortuitous circumstances of concentration, &c.; so that there may be new maladies awaiting our species, which are still to develop themselves under the endless combinations of the incidents of human life, through endless ages to come."

There can be no doubt that all specific diseases must have arisen from some cause and at some time, however remote. Of these some have now become extinct, and a few others whose existence was before unknown have made their appearance since the dawn of science; still the number of diseases recognized as really specific is very small, and it is remarkable that the fortuitous "combination of incidents" to which any one of them first owed its origin does

not appear, with any certainty, to have occurred a second time since the world began. If, however, for the sake of argument, it be admitted that circumstances have so combined as to produce any one of this class *de novo*, it must be allowed that it is of very rare occurrence.

If we take small-pox as an example of all the others, history almost forces upon us the conclusion, that, of all the myriads throughout the world who have suffered from this disease, not one has been so affected but by the propagation of new germs from the original virus produced in the body of some ancient inhabitant of China, centuries before the disease was even heard of in Europe. To say, then, that this scourge was utterly unknown in the western world before it was introduced by the wild tribes from the plains of Tartary, who added this evil to the desolation spread by their arms over the most civilized portion of the earth, is to express a great and most extraordinary fact. It announces that before that era countless millions of human beings, in almost every circumstance of life, from the pampered patrician of Rome to the lowest dregs of the populace—from the prisoner, immured in a noisome cell, to the active mountaineer or free wanderer in the desert—had all alike carried to their graves, unconsciously and unscathed, a certain something in their constitutions that was capable of being converted into deadly poison by one infected breath. It asserts that their bodies contained a material, of which the existence could not even have been suspected till its presence was evinced by the transmutation effected in it by the contact of a specific reagent.

Experience has proved that the special materials on which alone the several specific poisons have the power to act, though unknown as to their exact nature, are various, and frequently finite in the system; that the portion of the system capable of being so acted upon is generally of limited quantity, and not essential to existence, and that when once converted to a new and different condition, and expelled, it is not, for the most part, reproduced by the ordinary process of nutrition; that the severity of an attack is not in proportion to the quantity of the poisonous reagent introduced into the system, but to the quantity and condition of the material contained in it, upon which the virus is capable of acting. It is also known that there will be complete immunity

from attack, (whatever the quantity of the poisons introduced into the system,) if this *materies* have been expelled by previous action, and not reproduced; and if, by a rare idiosyncrasy, the body be devoid of this material.

That substances not necessary to life, and moreover capable, by simple rearrangement of their elements, of becoming absolutely destructive to vitality, should be thus combined with the essential elements of the body, is sufficiently extraordinary; but it is not less so that the destruction and extirpation of a material almost universal in the human body, should not be followed by any appreciable influence on the general health, after the dangerous fever that is produced by its formation, and which conduces to its elimination, is once over.

One of the most interesting illustrations of the agency of specific virus on its special *materies* is to be found in the discovery of Jenner,—that a particular poison taken from the lower animals, or rather human poison, modified by transmission through the constitution of the cow, is capable of acting on the matter upon which alone the small-pox virus can exert its power in such a manner as to convert and expel it from the system; or so to change its nature, that the subsequent specific agency of variolous infection becomes impossible, or greatly modified.

One of the best and most familiar illustrations of the law quoted from Liebig, by which the action of specific poisons may be explained, is the effect of adding active ferment to a saccharine solution that contains a small and definite proportion of mucilage. When fermentation is thereby induced, the gluten is acted upon, atom by atom, till the whole of it has become converted into new ferment; but when the whole of the gluten contained in the solution has been so converted, further production of the ferment ceases, from want of material to which the exciter can impart its own state of transformation. The parallel of this with exanthematous fever is exact. When the virus is introduced into the mixed fluid of the blood, all the *materies morbi* it contains is converted by it to its own condition, and the production of fresh virus only ceases after all that this exciter has power to act upon has been consumed. But, further, if a small portion of the gluten that is undergoing transformation be transferred from the first solution to another similar one, it will propagate the same action to all the

gluten it contains, and so will a small portion of the virus thrown off from the blood of one person and absorbed into that of another propagate its action to all the *materies* that is present in that body. The parallel, however, does not stop here, for if active ferment be thrown into a solution in which no mucilage exists, or from which the gluten has been previously exhausted, no new ferment will be generated ; so will the introduction of virus into the blood be of non effect on those who by idiosyncrasy are fortunate enough to be without its special *materies*, or from whose system it has been eliminated by a previous attack of the disease, and not reproduced.

As an example of the same action taking place in the living body, but reversed, we may instance diabetes mellitus, for there the exciter exists in the body, and the *materies* is introduced, whereas in specific disease the *materies* exists in the body, and the exciter is introduced.

When malt, which contains diastase, is bruised and thrown into a warm solution of starch, a rearrangement of the elements of the starch is immediately induced, and it is converted into sugar. In diabetes, any starch that is conveyed into the digestive organs is immediately similarly converted, probably by some diseased secretion of the stomach or bowels analogous to diastase, and capable of communicating to it this state of transformation ; the sugar thus formed becomes a source of irritation to the system when absorbed, and the kidneys are excited to increased action to expel it from the blood. But if the supply of this *materies* (fecula) be stopped, and only such aliment be taken as contains no particle of starch, the formation of this source of irritation is cut off, the sugar previously formed in the system is soon expelled by the appropriate emunctories, and the symptoms of the disease cease, while the secretion which acted as the exciter remains dormant and innocuous in the system.

The foregoing considerations will demonstrate how little there is of real distinction between contagion and infection, and that actual contact of particles, in the peculiar state of chemical activity, that is capable of communicating the like condition to particles of similar composition, is always necessary ; that it is, in fact, the same process, whether the contact of the exciter with the mixed fluid of the blood be effected by matter directly introduced into the blood by the lancet, or absorbed by the pores of the skin from contact, or

introduced by means of invisible particles, exhaled with the sufferer's breath, and absorbed through the delicate membranes of the lungs of others.

The various modes by which absorption of such matters may take place are sufficiently obvious; but those by which their elimination from the system is accomplished, after they have been formed in the blood, are somewhat more obscure.

The remarkable phenomena observed upon the skin in those diseases which are best known to all as contagious, (viz., the exanthemata,) together with the very obvious manner in which small-pox is communicated by inoculation, have naturally led the vulgar to regard the skin as at least the principal organ by which disease is propagated, or even as the actual seat of the disease, to the neglect of the less palpable means of infection, by the absorption of active particles of the exciter from the breath.

It wants, however, but little consideration to perceive that the poison of the exanthemata is not formed in the skin, but generated in the blood, and deposited in the skin. Many elementary poisons, such as mercury and iodine, which cannot possibly be formed in the skin, produce eruption. These are matters which, if imbibed in moderate quantity, are removed by the natural emunctories of the blood without disturbance; but if introduced faster than they can be so eliminated, are thrown off by a vital effort from the more vital organs, and deposited in this part of the system, where their presence can be tolerated without material injury, until time is afforded for their extrication by perspiration and by the other eliminating actions.

Eruption, then, commences subsequently to the formation of virus in the blood, and the deposition of the poison in the skin is solely the result of a protective febrile action excited in the blood-vessels; the virus then acts as an irritating foreign body in the skin, and requires for its extrication more or less of the self-same reparative action than is necessary for the extrusion of a thorn.

The pus of a small-pox pustule, therefore, is not *the virus*, but only the *vehicle* of its extrication, with which the exciter is mixed up, and the poison is no more entitled to be considered as originally formed in the skin than the urates (deposited by a similar protective action in a gouty joint) can be held to be generated in the great toe, or, than the pus, which we know to be formed in the

veins, in phlebitis, and so mingled with the circulating fluid, to be actually generated in the joints or cellular membrane in which we discover it deposited.

The excess only of the poison over that which the natural exhalations are capable of expelling is thus deposited in the skin, and it is probable that it is not till the other organs are overtasked that the skin ever becomes the temporary depository of the superfluity. Cases indeed not unfrequently occur of genuine scarlatina where the virus is removed by the emunctories of the blood, so *pari passu* with its formation in the system that no such accumulation in the skin takes place as is necessary to produce the characteristic effects of its irritation; but these are very different from those cases in which the eruption is correctly said not to come out, or has receded; such are evidences only of the want of that protective power of vitality necessary to cast the superfluity aside, not of the cessation of its formation. Pertussis is an example of the formation of a specific poison in the blood, of which the fever evinces intolerance in the system, and which we know to be at length eliminated from it; but here, although the skin is probably an active agent in its exhalation, it is either not made the depository of the poison, or at least not as an irritant,—indeed, recent pathological research seems to point to another portion of the body as the receptacle of this accumulation.

It has been remarked that the various special materials on which the different excitors of specific action operate, (although necessarily as various as the diseases produced by their conversion,) generally exist in the system in limited quantity, and are not reproduced; exceptions, however, occasionally occur even in those diseases which commonly appear only once in a lifetime, which prove that their *materies* is sometimes reproduced by nutrition; but it is evident that this reproduction is effected very slowly, as even in those exceptional cases, the virus of small-pox for instance, may be introduced some months after an attack without effect, and yet the disease be taken naturally at a later period.

But these exceptions to the general rule of the protective power of a first attack of an exanthematous fever against a second, rather aid than impede the consideration of those other specific diseases in which a first is no impediment to a subsequent infection, and in which the *materies morbi* is not finite, as in the others.

In some diseases, such as syphilis, the material acted upon appears to be constantly reproduced along with those which are more essential to organic life; and in syphilis, the poison generated is not of that violently irritating character which requires an intensely excited action of the powers of life (or fever) to throw it off upon the less important organs. The tissues are tolerant of it to such a point that its accumulation in them will destroy their organization, not so much by an effort to throw it off, as by producing an incessant irritation; yet even here we find the *vis vitæ* resisting the morbid influence in the more vital organs, and in them no evidence of disease is to be detected, even where the deepest seated bones have suffered.

This class of specific poisons has no natural means of cure, like the exanthemata, where the patient must die unless the system be immediately relieved from their irritating presence; yet experience has proved that syphilis, at least, is not beyond the means of artificial cure. The materials of its agency are not very rapidly reproduced; and it has been discovered, that by carefully restricted diet, it is possible so to reduce or cut off the supply, that for want of matter for the exciter to convert to its own poisonous condition, its action ceases, and that portion of virus which had been previously formed, will, in a few weeks, be so effectually removed from the system by the natural excretories of the blood, as to be no longer present to renew itself when its *materies* is again supplied by better nourishment. We have also ascertained that certain medicines or chemical reagents, such as mercury and iodine, possess the power of so combining with, or altering this material, as to nullify the operation of the specific poison on it, so long as their influence is maintained; and that by keeping up this action for a period sufficient to allow the whole of the virus to be eliminated by the natural excretories, the disease will be expelled. That it is not upon the virus previously engendered, but upon the *materies* on which the virus is capable of acting, that mercury operates, appears probable from the well known fact, that the most violent effect of mercury on the system is no more effectual in hastening the cure or preventing a relapse of syphilis, than the most gentle evidences of saturation of the system; whereas, if the exciter alone were acted upon, the larger the quantity of mercury infused into the blood, the more perfect and the more rapid would be the cure.

It is much to be regretted that the importance of this principle is not always sufficiently recognised in the treatment of the disease, and that the action of the medicine is not always sufficiently long maintained to give time for the complete elimination of the exciter; for if even a few germs be suffered to remain, they will certainly renew the morbid action as soon as the material is again supplied by nutrition in a condition amenable to its operation, and then there is danger of its becoming so modified by constitutional peculiarity as to be rendered more difficult to eradicate by means which would have been effectual in the first instance.

To pursue this subject into its more interesting details in other specific diseases, would lead too wide of the object proposed, and might appear to argue greater confidence in specific medicine than experience would justify. Still it is only upon the principle of the gradual conversion of a special material (non-essential and finite, or uneliminable and essential to life) that we can satisfactorily account for the observed phenomena of specific disease.

In so far as we have yet learned, hydrophobia must be considered as belonging to this subdivision, being a poison that, once infused into the blood, will gradually convert to its own occult condition some unknown material, normally supplied by nutrition, and which will slowly accumulate in the body till its quantity at length becomes sufficient to produce its terrible effects upon the nervous system.

Whether the means of arresting the action of this exciter on its appropriate *materies* shall ever be discovered in this dire disease, as in syphilis, is very doubtful, for it unfortunately presents no signs of its gradual accumulation till the quantity amassed suffices to prove rapidly fatal, and we cannot even guess whether its convulsions (or even those of pertussis) have any remedial tendency, such as to promote the final elimination of the poison, were life sufficiently prolonged.

Assuming, then, that the above facts are correct, we must necessarily limit the term "*specific*" to those diseases which are caused by the action of a special reagent on a special material that is combined with the living blood; and we must conclude that the *virus*, or special reagent, and the *material* of the body on which it operates, consist of precisely the same chemical elements in different states of arrangement; that simple contact of the virus will suffice

to induce its own state of transformation on the elements of this material, but being incapable of producing this effect on materials of different composition, it can never produce any form of disease but the identical one by which it had been itself produced, and rarely indeed, if ever, be produced by other means than by direct application of its specific virus; lastly, as their materials exist only in the living blood, all such poisons must have originated in the living blood.

If, then, a disease arise from any source but contagion or infection from a living being,—if its morbidic poison act on the organism of the body generally, whether on the blood or tissues, as a whole, and not on a mere adjunct,—if its action be confined to an external part only,—or if it be capable of communicating a different disease, or of acting on a different part of the body, so as to produce a group of symptoms that differ essentially from those presented by its immediate source;—there will be sufficient evidence that the disease is not specific.

It will readily be perceived that there are many diseases inducible by contagion or infection, which do not fall under the above definition. Of these, the most simple example is the dissecting room finger, or the hand of a cook wounded in trussing game too far advanced in decomposition. An organic poison, whose elements are identical with those of the blood and tissues, *but which is not formed in the living blood*, enters the wound, the hand inflames, the action induced in the wound is propagated up the arm, and the excited action necessary to resist this influence (or fever) is the consequence; yet this action, thus transferred from dead to living matter, may be reproduced in others by applying matter taken from this wound to a healthy abraded surface, or the exhalations from it may give rise to erysipelas or puerperal fever.

The difference between this poison and specific virus, therefore, is, that whereas the one is formed only in the living blood, the other may arise from dead matter—*that* operates only on a small portion or adjunct of the organism,—*this* involves in its action the whole or greater part of the organic tissues. The one produces by contagion or infection only its own special disease,—this will produce different forms of disease, according to circumstances; likewise, as the one cause affects only an unessential adjunct of the organism, which is capable of being eradicated without injury to the health, immunity

from subsequent infection may result from a first attack ; but in the other the great mass of the organic elements of the body are liable to be affected by the cause, and therefore one attack will exercise no protective influence on the system against future contamination.

The organic elements of the body in general being, then, considered as the *materies morbi* of a large class of diseases, it is necessary to inquire what the term *organic* expresses, and what are the changes which these elements are liable to undergo, and which require that vital effort to oppose them, and remove their products, which we term fever.

Every body that grows under the influence of life consists of a mass of chemical compounds, produced by certain affinities modified by vitality, and vital affinity disposes to combinations of elements that are accomplished with difficulty in the laboratory, for they differ essentially from the arrangements which the same elements would assume under the influence of ordinary or *inorganic* chemical attraction ; when, therefore, the influence of life under which they have been formed is withdrawn, the elements of organic compounds will again fall under the dominion of the inorganic affinities, their exceptional arrangement will yield to the more normal, and organic matters be resolved into inorganic.

If for a time the vital products retain the form impressed upon them by life, it is owing solely to chemical inertia ; for the attraction of their elements is generally so feeble after death, that when any circumstance occurs to commence chemical action in them, the progress of their decomposition becomes rapid, powerful, and difficult to be arrested.

This reductive action in animal bodies is putrefaction, and it not only propagates itself in the body in which it commences, but the contact of a very small portion of matter in this active state of decomposition will suffice to induce the same condition in other similar organic compounds.

Even without apparent contact, a piece of fresh meat suspended in the neighbourhood of putrid flesh will take on the same action, and become tainted in a few hours ; the microscope, however, informs us that the contact in this case is real, as the atmosphere surrounding a putrescent mass is observed to be charged with minute particles in a state of active decomposition, capable of being conveyed to a great distance.

Putrefaction, or organic decomposition, therefore, is that chemical force which is most directly opposed to the formative power of vitality.

But although the powers of life are directly opposed to those inducing putrescence, it does not follow that putrefactive action can produce no effect upon the living blood. Liebig has well remarked, that "No other component part of the organism can be compared to the blood in respect of the feeble resistance which it offers to exterior influences, for it is not an organ formed, but in the act of formation: the chemical force" (or tendency to inorganic combination) "and the vital principle hold each other in such perfect equilibrium, that every disturbance, from whatever cause, effects a change in it. The blood possesses so little of permanence, that it cannot even be removed from the body without suffering immediate change, and cannot come in contact with any organ in the body without yielding to its attractions. The slightest action of a chemical reagent exercises an influence upon it; even momentary contact with air, through the medium of membranes, alters its colour and other qualities; and every chemical action, such as the active condition of the constituents of a body undergoing decomposition, fermentation, or decay, when brought into contact with the circulating fluids, disturbs the equilibrium between the chemical force and the vital principles, and the action will propagate itself through the entire mass of the blood."

These extracts will suffice to show what dangers we should hourly incur by breathing tainted air, but for the influence of the opponent power of vitality; and that our bodies, like fresh meat exposed to the exhalations of putrid flesh, would soon be reduced to a mass of gangrene by the absorption of putrid exhalations. In truth, we are apt to forget how much we are indebted to the preservative powers of life for the resistance offered to this influence, and the removal of such noxious particles from our blood, till our attention is awakened to it by observations like that of Dr. Angus Smith, who found the condensed vapour on the windows of rooms occupied by crowded assemblies, to be always charged with animal matters in a state of most active putrescence, that had been exhaled from the bodies of the multitude.

It would appear, then, that putrefactive particles may continue

for a long time to be absorbed into the system, and be either changed by the antiseptic chemistry of life, or thrown off from the body in the same state in which they were received, without their having attained sufficient power to induce their own state of transmutation on the blood. There is even reason to believe that this power of resistance may be increased by habit, as in the case of night-men and the cleansers of sewers, &c.; still this immunity is seldom quite complete, and the operation of an anti-vital cause is exhibited in the earthy cachectic hue and spongy gums, and the mental depression remarkable in the inhabitants of the worst quarters of our towns.

When the body has thus been subjected to the influence of putrefaction, removal to fresh air, and improved diet, will soon restore the vital powers to vigour; but just as good air and food will aid the powers of life to overcome the anti-vital tendency to decomposition; so will anything that may occur to depress the vital powers give ascendancy to their opponent. Loss of blood, debauchery, cold, or deficient nourishment, may each produce this effect, and then, instead of being exhaled or destroyed by the vital chemistry, the decomposing agent will become active on the organism, and give rise to the actual formation of compounds in the blood that are incompatible with life, and which must either be removed, and their action arrested, or must go on increasing till life is overcome.

But the body is wonderfully endowed with sensations to warn it of danger, and to rouse up the vital energies to resistance. The presence of this irritant excites the powers that circulate the blood to increased activity, to resist and remove this cause of injury; and this reparative action, in the case in question, is the simple typhoid or low putrid fever, endemic in our towns—a *mere struggle of the vital powers against the tendency to organic decomposition.*

So long as this struggle lasts the powers of life must labour not only to form healthy organic compounds from the blood, but to throw off the putrid particles actually formed in the body. The exhalations, therefore, from a person in this state of fever are necessarily highly charged with organic particles in the most active state of putrefaction, and any one who breathes an atmosphere thus loaded, puts himself in the same relative position to the patient as a piece of fresh meat will occupy when suspended near putrid

flesh ; as surely as the meat will yield in a few hours to the power of chemical decomposition so induced, will his body become the subject of putrid fever, unless protected by great resistance of the vital powers.

Of this I shall offer an illustration. A young man, living in a healthy out-district of Manchester, in which no fever at the time prevailed, was employed in the manufacture of phosphorus matches. His health was apparently unaffected by the putrid atmosphere in which he worked, until one day the manufactory took fire. After making great exertions to extinguish the flames, he returned home wet and much exhausted ; he was seized with rigors, which were immediately followed by highly putrid fever ; he was covered with vibices, and died in a few days, with extensive gangrene of the intestines. The powers of life had previously sufficed to expel or neutralize the putrid particles inhaled in his unhealthy occupation, till cold, wet, and exhaustion combined to depress them ; but no sooner were they thus temporarily impaired, than the anti-vital influence of putrescence became paramount, and the blood, as well as the tissues of the intestines, became subject to its destructive action. So long as the struggle was maintained by life, the active putrefactive particles thus formed in his body must have been exhaled by the excreting organs infecting the atmosphere ; by these means his body became a focus of disease, and the immediate neighbours first, and then those most intimate with them, became affected with fever. All the earlier cases of fever induced by this contagion were complicated with derangement or ulceration of the bowels, as if this peculiarity had been communicated along with the infection, as much as the eruption of measles or scarlatina follows contagion of these specific fevers ; but it soon appeared that this was not the case, for very shortly, cases of fever distinctly traceable to the same source, had the complication transferred to the lungs, and instead of ulcerated bowels, there was pneumonia as an almost invariable concomitant ; at the first, diarrhoea and dysentery had been prevalent in Manchester, but just at this period the epidemic changed to influenza, and the fevers all over the town became more or less associated with pectoral derangement.

Here, then, was a fever which there were reasonable grounds for believing had arisen from an external cause becoming communicable, but not always producing the same form of disease, for at

first it was characterised by abdominal, afterwards by pulmonary, inflammation. In the first instance, too, so far from confining its action to a mere adjunct of the organism, its exciter affected the whole mass of the blood, and all the tissues of a portion of the intestine, evinced in the petechiæ and sphacelus; therefore the contagion was not specific. The complications of ulcerated intestine and pneumonia in the subsequent cases were the effects of epidemic influence superadded to the putrid fever; but if, instead of epidemic influence, specific poison had been applied to a body in this condition, the effects produced would have been more obvious, and might have been more decidedly traceable to a superadded cause.

It does not want much argument to show that any morbid cause, whose operation on the system is only opposed by the resistance of the powers of life, will be greatly promoted in its action by any circumstance that can depress them; but when the cause of this depression is the previous establishment of an anti-vital chemical action in the system,—which is a permanent and increasing, instead of a merely temporary, opponent to the powers of life,—it will not only promote the effect of that other morbid influence, but will itself be rendered more active by the combination.

Of the combination of the specific with the putrid causes of fever we have had abundant experience within the last two years of pestilence and famine, in the almost universal diffusion of specific eruptive typhus, imported from Ireland, as it occurred in a cellar population. Day by day thousands of the inhabitants of that unhappy country sought refuge from starvation by abandoning their homes, and after having been huddled together on board ship, arrived in Liverpool or Glasgow in the last state of destitution. There, seeking a temporary asylum in the over-crowded lodging-houses, that, even at the best of times, are notorious hotbeds of putrid fever, or in damp noisome cellars and abandoned dwellings, it was almost impossible that any one previously intact could escape contagion. An atmosphere akin to that of the gaol fever of the Black Assizes was engendered; and when these unfortunates took their way into the interior, to beg or earn their bread, they left the way literally strewed with dead, and a legacy of pestilence to all who ventured to extend to them the hand of charity, until at length this contagious and specific fever became disseminated in

nearly every town and village in Great Britain, and added its special cause to those of putrid fever which had previously existed.

Few portions of medical history are more complete than that of Irish typhus. Time immemorial rooted in that country, and never extinct, it has always been ready to break out afresh on the occurrence of any favouring influence of whatever kind, be it the depression produced by famine or by epidemic causes; and then, after the first outbreak, it has seldom ceased its ravages till it has extended itself over the whole island. A tendency has been remarked in this peculiar fever to assume the form of pestilence at intervals of about ten years, or—to use the ordinary but misapplied expression—to become epidemic. The Board of Health attributes this peculiarity solely to epidemic influence; but this is certainly a fallacy, easy to be disproved, did time permit, by a review of the various well-recorded complications which it has manifested in this and the last century in Ireland: besides, the true specific nature of eruptive typhus is now established beyond all doubt, and the immunity conferred by one attack against a subsequent infection is as fully proved as that of measles, small-pox, or scarlatina. The fact, then, is incontestible, that, whether by inhalation or contact, contagion is the one and only means by which this disease is propagated; and though its diffusion may be favoured by atmospheric or other causes, it assuredly does not depend “on certain atmospheric conditions,” either for its production or diffusion, as the authors of the Report on Quarantine assert.

The decennial recurrence of typhus as a pestilence is only what used to occur in small-pox, at longer intervals, and is what must naturally be the case with all highly contagious disorders which only attack once in a lifetime; it therefore admits of simpler and more intelligible explanation by this peculiarity, than by the very problematical periodical return, at such intervals, of an unknown and improbable condition of the atmosphere.

If immunity from subsequent attack be secured by having once undergone the perilous process necessary for the elimination of the *materies* of specific typhus from the system, and if nearly every one (whose circumstances subject them to communication with the infected, and to the same depressing effects of lack of food and unwholesome dwellings,) have attained this immunity, the propagation of the contagion must almost cease, and, but for the occurrence

of a few cases here and there among those who had previously escaped, it might be supposed that the disease had actually become as extinct as small-pox is said to have been rendered in Sweden by the enforcement of universal vaccination. Experience teaches us that typhus does not so readily affect children as the other exanthemata, and that it is rather a disease of middle age; but by the time that ten years of incomplete cessation have elapsed, children have grown men, many country peasants have become towns'-people, while others whom better circumstances had contributed to preserve before have now sunk into poverty, and thus fresh food for this contagion is prepared in persons of all ages. In years of plenty, vigorous and well-nourished constitutions will resist a dose of this contagion that would suffice to prostrate those whose blood has been impoverished and vital powers depressed by deficient and unwholesome food, and in such seasons of universal famine as the last two years, few who have not previously been infected can possibly escape contagion under such wretched circumstances. It must be remembered, too, that even those who had before suffered from specific typhus, still remain subject to putrid fever, and may receive its infection from those in whom the putrid and the specific fevers are combined, as well as through the influence of the endemic causes which surround them. Hence the recent universality of fatal fever in Ireland.

When famine became general in Ireland, in 1847-8, a longer period than usual had elapsed since typhus had universally prevailed in that country. In most towns in England the disease was rarely met with; and it was remarked in Dr. Davis's Essay, that not a single case of maculated typhus had been observed in Bath for years; also, that when at length it did occur, and its contagion spread, he was able to trace its introduction distinctly to an individual. But, although specific typhus had been of late years rare, the ordinary typhoid fever endemic in our towns had always, more or less, prevailed. In 1844-5, and 1846, among many hundred cases of fever visited in dispensary practice, in the out districts of Manchester, I did not meet with a single case of specific typhus—all were either the epidemic fevers of the period, or these mixed up with the putrid fever of ill-drained and crowded habitations. In the spring of 1847, the failure of the potato crop of the preceding year, and the great depression of manufactures, which

threw thousands of work-people out of employment, combined to render land-scurvy deplorably prevalent; and the *famine fever*, or lowest form of petechial typhoid, became very frequent; but except in the two quarters of the town, inhabited almost exclusively by Irish, there was absolutely no eruptive typhus in Manchester; and even there it was as yet infrequent. This, however, did not long continue to be the case. Every wretched and over-crowded lodging in the Irish quarter was soon filled with infected families from Ireland, and became a focus of disease, and then the two causes of fever, in combined operation on the same individual, were manifested in the great majority of cases, for the specific eruption appeared mixed up with the petechiæ of putrid fever.

After this typhus rapidly spread into other districts. The multitudes attacked in the end of 1847 and beginning of 1848 were countless, for of the whole native population of Manchester scarcely one had acquired immunity from contagion by previous attack. It was not till the autumn of 1848, when most of those exposed and liable to the disease had passed through the infection, that specific typhus rapidly decreased; and since the commencement of the present year, (1849,) the number of patients admitted into the Manchester fever wards has gradually fallen to its yearly average. Of this number, only about a third are now (July) cases of true specific typhus, the others being of the ordinary typhoid of the town, modified by epidemic influence, or of that low congestive form of fever, afterwards to be described, which belongs to the reigning epidemic constitution. Of these latter, however, almost all have previously suffered within the last two years from specific typhus; if not, they are in danger of contracting it in hospital, as happened not unfrequently when it first began to prevail, and the attack was often considered as a relapse of the previous typhoid. Suffice it, then, to say that typhus spread till all who were exposed to its contagion, and who had not been previously affected, had taken it, and then it very rapidly decreased.

But the Board of Health has bound itself down to the assertion (page 5) that "Typhus, scarlatina, influenza, plague, yellow fever, and cholera, are all dependant on certain atmospheric conditions, and all obey similar laws of diffusion;" in other words, that all fevers, whether of specific or other origin, are propagated solely by epidemic influence. At page 6, this view is intended to be still

more explicitly expressed, by stating, "That without the presence of an epidemic atmosphere, no contagion can cause a disease to spread epidemically."

Not to dwell upon the only sense in which we can concur in this opinion, viz., that unless a disease be epidemic, it is not epidemic, let us inquire how far typhus, (and, of course, all other diseases of specific origin,) are influenced in their production and diffusion by an epidemic atmosphere. The experience of history, and of almost every medical man in England, has proved that without specific contagion, neither filth, crowding, nor epidemic influence, nor all these three combined, will suffice to produce either typhus, scarlatina, measles, or any other specific disease; yet we cannot doubt that each of these causes may act as a depresser of the powers of life, and so promote the action of specific virus when applied. Perfectly disposed to admit that certain atmospheric conditions may, and often do, depress the vital powers of resistance, so as greatly to promote the action of other morbid causes, our experience goes to prove, that deficient nourishment and unwholesome dwellings have been the main, if not the only, causes that have contributed to promote the diffusion of typhus and scarlatina in England during the last two years of pestilence; and we do not find that any satisfactory reasons have been shown by the Board of Health for believing that any particular state of the atmosphere must necessarily concur with so competent a cause for their diffusion, as the introduction of active specific poison among a dense population universally liable to its action, which moreover was, for the most part, predisposed to its reception by poverty and insufficient food. It is only in the vulgar sense that typhus, scarlatina, small-pox, measles, &c., can be said to become epidemic; they are specifically contagious diseases, not epidemics; and so far from obeying the same laws of diffusion with influenza, plague, or cholera, as the Board says they do, they obey laws of diffusion peculiar only to diseases which have no origin but contagion, and only occur once in a lifetime, spreading so long as any who are susceptible to their contagion continue to be exposed to it, but no longer, whatever the nature of the epidemic atmosphere. Still specific fevers are not wholly uninfluenced by epidemic causes. The very derangement of health, caused by contagion, renders the whole

system more amenable to epidemic influence, and wherever such influence exists, it will more or less modify the course of any individual case of fever.

We have endeavoured to define epidemic influence according to its literal meaning, as a superimposed cause of disease, one which produces no propagable action on the body, but which, acting like some inorganic or vegetable poisons received into the blood, or by means of some unknown external agency, such as electricity, affects a portion of the nervous system, and thereby produces disturbance of some particular function or organ. It remains, then, to enquire how far such influence has been manifested upon fever in the course of the last two years in which the specific pestilence of typhus has been so widely spread.

In the early part of 1848, when specific (improperly termed epidemic) typhus was at its height in Manchester, the town was even more than usually free from all signs of epidemic influence, and fever displayed no remarkable tendency to complication with disorder of any particular organ. A large proportion of cases from the worst neighbourhoods and cellars, it is true, exhibited the influence of the endemic or putrefactive causes in combination with the specific contagion, as shown in the admixture of petechiæ with the true papular eruption of typhus, and in the frequent occurrence of extensive gangrene in the neck of children affected with scarlatina, for we know that petechiæ and gangrene, though they belong to the worst forms of typhoid or putrid fever, are not in any way essential to either typhus or scarlatina. Still there was remarkably little tendency to any other kind of complication or diseased action of any organ of the chest or abdomen, or of the brain, such as we always perceive where epidemic influence is active, and which must have been the case had the Board been correct in attributing the diffusion of these specific fevers to "some peculiar condition of the atmosphere."

In June, however, a really epidemic influence was manifested. Dysentery became prevalent in the town, and every case of fever was then complicated with derangement or ulceration of the bowels. This ceased in a few weeks; but in August, the remarkable tendency to venous congestion, which always accompanies the epidemic influence that belongs to cholera, began to be felt, as the disease approached our shores. Throughout September the cases

admitted were chiefly of a low congestive character, with mottled chilly skin and black tongue, with small contracted pulse, and tendency to morning remission, exhibiting great disposition to relapse. The cases of specific typhus showed less of these characters, probably in consequence of the excitement produced in the arterial system by the presence of the specific irritant, but they also were generally complicated with severe congestion of the brain and portal system. At the same time every case of scarlatina indicated the same congested condition of the kidney by the increased frequency with which the attack was followed by albuminuria and dropsy, and by the greater severity of these symptoms.

In December this feature changed, influenza became prevalent, and then almost every case of fever, of whatever kind, was complicated with bronchitis.

In May and June this again gave place to the same congestive fever that had been observed in September, and at both of these periods a few cases of cholera occurred.

Such, and such like, are the only true effects of an epidemic atmosphere or influence on specific fever, and such effects are always even more evident on fever which has its origin in putrescence without specific intermixture.

If, then, at any time we observe specific fever to be accompanied with the petechiæ and tendency to gangrene that belong essentially to typhoid fever, we may consider both causes in operation on the system; and when, in addition to the characters which we know belong only to the specific disease in its pure state, or to the typhoid form of fever, we observe a general tendency to affection of a particular organ, or of one part of the system more than another, we are entitled to consider this complication the result of epidemic influence superadded on the other causes of disease.

This will become the more evident if we also observe that the organ chiefly affected in fever is that which, for the time being, is most liable to derangement in those who are merely attacked with cold, &c., without other cause of fever. Every practitioner knows that when slight causes of depression, such as cold, are succeeded by reaction, and inflammation of the throat, the lungs, the pleuræ, the bowels, or of any particular organ in one or two persons at the same time, he will probably soon meet with other

cases of the same character; and he expects that when he goes into the fever wards he will find more or less complication from disorder of the same organ in almost every case. These are the results of epidemic influence, and such complication affords the most legitimate means of studying the effects of these causes. It was by the attention which he bestowed on such complications that Sydenham did so much to improve his profession; and there certainly does not exist a subject of greater interest in the whole wide range of medicine than to observe, and carefully note, the extraordinary multiplicity of functions and organs that are thus, month by month, and year by year, affected by the epidemic influence. Now one set of glands in the intestines will be affected, now another. Now the lymphatic system, as in plague. Now the skin, as in sweating sickness, erysipelas, &c. Now the influence will attack the joints and muscles, with rheumatic inflammation, or the serous membranes, by producing pleuritis, peritonitis, meningitis, endo and peri-carditis. Now it will disturb chiefly the arterial system, and cause excitement and active inflammation; or the venous, producing disorder of its functions in agues, cholera, remittents, and passive congestion of various organs, such as the lungs, the spleen, the brain, the liver, or the kidney.

Suppressio urinæ, diuresis, the formation of oxalates, vomitus, diarrhœa, constipatio, icterus, spasms, convulsions, neuralgiæ, paralyses, &c., &c., are all on record as the effects of disturbance of the various vital functions and organs produced by epidemic influence; and even those still more unintelligible functional disorders of the brain that result in strange psychical derangements, evinced in the dancing mania and tarantulism.

The subject is too vast to be more than hinted at; but infinite as may be the variety of causes to which the great epidemics of history have been attributed, such as tides, winds, mists, earthquakes, volcanoes, comets, electricity, and every mysterious cause included under the term "divine wrath," it will be found that the effects of epidemic influence on the human constitution are even yet more varied. It is possible that all these causes may ultimately be resolved into three, viz.,—poisons floating in the atmosphere, poisonous ingesta, and the disturbed balance of the electricity of our bodies in reference to that of the earth or air; but such speculations will avail us little. In the meantime

we have rather to do with effects than causes, until, perhaps, advancing science may one day permit of general laws being deduced from the accumulation of well observed facts. Practically we are as yet only entitled to consider epidemic influence as a cause of disease that will manifest itself for a time in morbid derangement of some function or class of functions, and so render some organ or class of organs more than usually prone to disease; that will extend itself over large districts in the course of a few hours, under circumstances for which no probable theory of contagion will account, and as one that is in no case capable of directly inducing propagable morbid action in the system, or of converting any part of the elements of the body to its own condition, so as to render that body a focus of disease; for this is the only mode in which it appears possible for any disease to become contagious.

But although not capable of regenerating itself in the body, and so becoming communicable, the *epidemic influence* is capable of giving activity to other morbid causes previously dormant in the system, whether specific or putrefactive, by the depression which it produces on the powers of life. It will thus aid in the production of propagable disease, which it cannot itself originate, and will moreover give to such diseases a pseudo-specific character, by superimposing upon them the peculiar derangement of function, which it is its province to produce.

It appears to be only thus that the incessant variations of our towns' fevers can be accounted for, from month to month or year to year, or, we may add, from era to era.

Each man in his generation is witness to many such changes in the character of disease; but it is only from history that we learn that such are but the minor phases of great epochs of disease, of which the grander features imprint their general character on the less.

At the commencement of this century highly inflammatory affections were predominant; but since 1828 the lancet has been little used, and every kind of tonic treatment has come into vogue, yet we see that even in the most ancient times such changes took place in the usual plans of treatment. Galen blames Hippocrates for neglecting to mention the use of venesection in a disease where he did not venture to doubt that it had been employed, because he

himself had found it necessary. The innumerable commentaries on these works bear testimony to this ever-varying character of disease; one learned writer, judging from his own experience, accuses the great father of physic of error, while another, in a different generation, and for similar reasons, condemns the commentator, and pins his faith to the original. The change of the means of ministering to disease, so as to adapt the remedies to the exigencies of the period, is too often attributed to fashion only, or to fanciful theory, when it really is due to each man's experience of the necessity of altering his line of defence to meet a changed mode of attack by the disease. In general, change of *fashion* in the *treatment* of disease is evidence complete of essential change in its *character*, and is not the effect of doctrine, though new doctrines are often concurrently propounded in consequence of the effects of a temporary cause being mistaken for the *cause* itself. Thus Broussais, finding every fever he saw complicated with intestinal ulceration, taught the continental physicians that this ulceration was the invariable cause of all fever; but Broussais' authority has already passed away with the gradual change that has occurred in the type of fever; so every well-established change in medical treatment and most fashionable doctrines will mark an epoch of disease. Thus, to posterity, the general belief in homœopathy and infinitesimal doses of the present day, will serve as evidence of the long existence in this century of a peculiarly inactive character of disease; for we know that both the doctrine and the practice of Hahnemann must cease to be fashionable so soon as a more inflammatory character of disease shall reappear.

Extended examination of the subject will, I think, leave no doubt that any cause of fever, but more especially the putrefactive causes, will render the system more amenable to any epidemic influence that prevails. Thus Dr. Christison's cases, cited by Dr. Davies in his Essay, though arising only from local exhalations from putrid manure, assumed the most aggravated form of that type of fever which then prevailed throughout the country, known as the Edinburgh epidemic of 1843. Had plague then prevailed, these would have been cases of plague, with all its characteristic symptoms, just as putrid cabbages gave rise to the first cases of plague in Oxford. If cholera had been epidemic, these persons would have suffered from cholera; but as it was, the disease was a congestive remittent.

If, then, epidemic influence manifests more or less of its effects upon the system in all fevers, may we not hold it as a corollary that when an infectious fever, complicated by such influence, communicates itself by contagion to one who is not within the influence of that epidemic cause, it will be conveyed to him without these effects?—in other words, that the contagious fever will then be propagated in its simple form, without one of those symptoms that had been superadded upon it in the previous case by that cause. This is the point upon which the Board of Health ought to have concentrated the evidence, in order to prove quarantine misapplied to epidemic disease.

If, instead of considering the diffusion of all diseases to depend upon epidemic influence alone, and denying to contagion any effect whatever, they had admitted it as a settled point that small-pox, and all other diseases of specific origin, may be excluded from a country by an efficient system of quarantine, they would only have acknowledged what is fully proved by every species of evidence, direct and indirect, and equally consistent with experience and theory, and then there would only have remained to the commissioners to consider the not very difficult question of the expediency of maintaining an establishment to exclude what was already in the country, at least in so far as this particular class of diseases was concerned. But with respect to plague, cholera, influenza, and yellow fever, abundant evidence exists in our own and the French reports, and elsewhere, to show that these are not specific but epidemic diseases, that their peculiar symptoms are produced only by epidemic causes, and that although, where their epidemic cause exists, they may sometimes appear to spread by specific contagion when they are mixed up with contagious putrid fever, it is the typhoid fever that is so communicated, not the epidemic symptoms. That if a fever so combined with plague, infect any one who is beyond the reach of the epidemic cause, the fever is communicated without the plague symptoms, for the cause which alone is capable of producing these symptoms is absent. There is, also, evidence to show, that wherever this epidemic cause exists, its peculiar symptoms will immediately be superadded on the endemic fever of the country, quite independently of all contagion; and, therefore, though sanitary measures, directed to remove the causes of *endemic* fever, will

avail much to diminish the fatality and curb the extension of such pestilences, both quarantine and sanitary measures are utterly powerless to prevent their introduction into a country.

All this might have been proved by evidence, without going beyond a simple statement of facts. It is substantiable that a ship sailing from Alexandria with a crew infected with plague, only carries the contagious fever *as plague* to the limits of the epidemic influence; and that even if the contagion does propagate itself in a foul, ill-ventilated ship, beyond the reach of that influence, it is only as putrid fever that it does so, and the only disease it can communicate to a country to which that epidemic influence has not extended, is the identical form of typhoid that was previously endemic in that country, without one single attribute of Egyptian plague. For example, while I was resident in the capital of Persia, a body of cavalry came under my care, after a rapid march of two hundred miles from the district of Khūmsa, where plague at the time prevailed, and where every one attacked with fever was affected with plague buboes. These men brought with them an aggravated form of typhoid fever; but after their arrival at Tehran, though most of them were affected with petechial fever, and two died, not one of them had buboes. In Tabreez, too, which I entered with Sir Henry Ellis's embassy, in 1835, only four or five days after plague had suddenly ceased in the town, I found cases of typhoid running their regular course without buboes, although said to have taken the contagion from plague patients who had had buboes. If, however, a ship leave Alexandria with plague on board, and cases manifesting plague symptoms are actually in it at the time of its arrival in a port, it is evident that she has never quitted the range of the epidemic influence, and that this influence has either travelled *pari passu* with the ship, or has preceded it; and, in all probability, this influence had already been manifested by superimposition of the epidemic symptoms on the typhoid fever of the worst districts of that port or neighbouring town before the ship's arrival. Such would appear to have been the case in the often quoted instance of the commencement of cholera in the Isle of France, a few days before the *Topaze* frigate arrived with cholera on board from Ceylon. But when it happens, as in the case of the *Eclair*, at Boa Vista, that the epidemic influence

does not reach a port till just after the arrival of an infected and very foul ship, and almost immediately after communication with the ship a pestilence breaks out, the connection between cause and effect appears so decided, that those who attempt to argue that *post hoc* is not necessarily *propter hoc*, are scarcely listened to with patience by the generality of mankind.

With regard to Egyptian plague, it is, indeed, rather remarkable to those well acquainted with its habitats, that although it has always been the principal object of quarantine, it presents infinitely more evidence against, than in favour of, the supposition that it can ever be propagated beyond its own limits by contagion, or become contagious in its own essence. Whatever the cause of other epidemic diseases, it is pretty evident that that of plague depends on the admixture of the winds from tropical Africa with the colder breezes of the north, as it always makes its first appearance where these winds meet and form clouds.

In Egypt, plague never precedes the southern breezes in spring, and it ceases there after that wind has become dominant and unmixed, in June. It does not then recur till the north wind again approaches the Egyptian coast, in November; and as that wind in its turn acquires the ascendancy in the winter season, plague again ceases till the following spring. Each spring and summer, plague accompanies the extension of the tropical wind towards the north, and each autumn and winter, it recedes towards the south-east, as in their turn the wintry blasts prevail and drive the point of admixture back towards the south, till it reaches Upper Egypt, where earth and air are so dry that no moisture is precipitated where they meet, no clouds are formed, and no plague is induced. Its ordinary course of late years has been as follows:—It first appears in Egypt in March; in Cyprus in April; in Smyrna in May; in Constantinople in June; at the mouths of the Danube and Odessa in July and August. Towards the Equinox, the wind becoming more westerly, plague attacks the east coasts of the Black Sea, Samsoon, and Trebizonde, and the high grounds of Armenia, from Erzeroom to Ararat, where the south winds mix with the breezes from the Caspian. As winter begins it falls back upon Damascus and Aleppo, reappears for a time in Egypt, and settles upon Mecca, and then in the next year, in March, it begins its course anew. This course doubtless

often varies, sometimes spreading far into Asia, sometimes overwhelming even the west of Europe; but in these wanderings the principle appears to be maintained, as it is said that in the years that it extends farthest into Europe, Asia is free, and *vice versa*.

To any one who has experienced the depressing effects of the plague atmosphere, it is easy to understand how its advent will act upon a system prepared for disease by the filth of an eastern city. Every one attacked by, or even subject to, the cause of putrid fever, becomes in a few hours a mass of putrefaction where the plague influence exists, and, covered with vibices and gangrene the patient presents one of the most appalling spectacles that can be imagined. The putrid exhalations that then emanate from the bodies of the sick, rapidly bring others under their influence and that of the epidemic, and then, indeed, is plague, or at least the putrid fever with which it acts in concert, fearfully contagious.

Thus as it proceeds in its annual course, so soon as the epidemic influence is felt, the endemic typhoid of every town it reaches becomes contagious plague. Nor are those exempt who have no putrid nor specific cause of disease in their systems. If depressed by previous illness, cold, or famine, they are affected with great mental depression and with colliquative sweats, and buboes appear in different parts of their bodies; but there are no pecthiæ or other putrid symptoms, and these persons may mingle freely with others, without the slightest chance of conveying to them the disease. Evidence to this effect is very abundant. Such was my own case, and my servant's. In March, 1840, we sailed down the Nile on the verge of the first southerly wind, all the way from Thebes, and having thus been more than usually exposed to the concentrated cause of epidemic plague, slight exposure to wet in the Mahmoodean Canal, sufficed to bring us under its influence, and we were attacked in Alexandria with buboes and exhausting sweats; yet we lived with thirty others, on board ship and in quarantine, for upwards of three weeks without communicating the infection to any one.

It is thus only that an observation I have frequently heard from the best informed of the medical men whom I met in Egypt can be explained. They assert that plague caught by contagion is always contagious; but sporadic plague, not received by infection, is not infectious, and this I would interpret to mean,

that when combined with putrid fever it is catching, but unless combined with contagious disease is incommunicable.

The three causes of disease usually called epidemic have now been sufficiently dwelt upon to show that the *specific* is always contagious; that the *endemic*, or putrid fever, may be contagious, but may also arise without contagion; and that the *epidemic* is never contagious in itself, although it may appear to be so when mixed up with the other two.

In reference, then, to the opinions advocated in the Government Report on quarantine, though we may agree with them that plague, cholera, influenza, and other epidemic diseases cannot possibly be communicated by contagion or fomites, we cannot concur in the belief that—"Without the presence of an epidemic atmosphere, no contagion, whether imported or native, can cause a disease to spread" as pestilence; for every evidence goes to prove that specific disease diffuses itself quite independently of such influence. We object, too, to the statement that—"The notion of the propagation of plague by means of goods appears, from one uniform mass of evidence, to be as entirely unfounded as the opinion which formerly prevailed in this country, that typhus could be propagated in the same mode;" because if this were true, and plague were as communicable as typhus by means of goods, furniture, clothes, &c., we must consider it to be like typhus—a specifically contagious disease—and assert the necessity for maintaining quarantine to oppose its introduction, which is exactly the opposite to what the Board wishes to inculcate in this sentence. So far from this being the case, however, we believe quarantine quite useless against plague, though had it been possible to maintain an efficient system of quarantine against the introduction of specific typhus from Ireland we cannot doubt that very many thousand lives would have been thereby saved in Britain during the last two years.

So far, then, from acknowledging the Board of Health to be in advance of the medical profession in their knowledge of the causes by which disease is produced and propagated, we cannot but consider the commissioners in retard of the present state of information on the subject when they declare that—"Typhus, scarlatina, influenza, plague, yellow fever, and cholera, all obey similar laws of diffusion." It is, however, only justice to the Health of Towns,

and to the Board of Health, Commissioners, to acknowledge that we are indebted to them for an immense amount of most valuable practical and statistical information, and that legislation founded upon the recommendations contained in their Report on quarantine is calculated to do away with enormous abuses, cruelties, and injury to commerce, and likely to be attended only by inestimable advantages to the country. Against their practical conclusions there is nothing to be urged, but to the principles which they have propounded, as the very basis of their Report, we must object *in toto*. They have maintained epidemic influence to be the only cause of the diffusion of fever, &c., whereas we have endeavoured to demonstrate that this depends on three distinct and different causes.

We believe that all SPECIFIC diseases are propagated only by contagion, and that quarantine and isolation of the sick is the only effectual bar to the introduction and diffusion of such diseases; but as nearly every known form of specific disease propagable in this climate already exists in Great Britain, it would, at present, be absurd to maintain quarantine for this purpose, though removal of the sick ought to be more insisted upon.

We believe that all diseases properly termed EPIDEMIC are in themselves non-contagious; that their cause does not emanate from the bodies of the sick, and is not capable of attaching itself to clothes, goods, ships, or places, but that it is a pervading external influence that extends itself uncontrollably over the earth, generally or partially,—is governed by unknown laws, and affects the health through various media; that, therefore, quarantine and isolation are wholly inoperative to prevent the introduction or diffusion of diseases of this class.

We believe ENDEMIC fevers to arise chiefly from local causes, all of which are more or less preventible, and that they are capable of becoming intensely contagious—that their most fruitful cause is respiration of air loaded with putrid exhalations from mixed masses of animal and vegetable substances in a state of decomposition—their most contagious source, putrid exhalations from the bodies of beings crowded together in a confined atmosphere, such as our living gaols once enclosed, and those from persons actually suffering from putrid disease—and their most poisonous source, putrescent viands, bad meat, and sausages made from the flesh of over-driven animals, &c. &c.

But as the causes of endemic fever include every circumstance by which the tendency to chemical decomposition is promoted or induced in the living body, such as unhealthy occupations and habitations, bad air, bad food, filth, want of drainage, water, or ventilation, and as these are causes that exist almost everywhere, it is vain to think that diseases which directly arise from them can be excluded by quarantine or isolation, or that their diffusion can be prevented by any practicable means but those which are now actively in progress for the improvement of our towns.

Such, then, separately considered, appear to be the three principal causes of the more widely spread diseases that affect communities, and such their characteristics as regards their communicability from man to man; but we have endeavoured to show, that in densely inhabited neighbourhoods these seldom operate upon the system singly, and that all three frequently unite in the production of disease, and by their combination that each promotes the diffusion of the others; also, that to this admixture of causes is owing the extraordinary variety in the types of fever with which we have to contend. In the example of typhus, we have seen how specific disease may be modified by endemic and epidemic agency, and in that of plague, how a non-infectious epidemic disease may appear to acquire the virulently contagious properties of specific fever, and become the putrid pestilence that it once was in London. In the fevers of our towns we have seen how maladies purely endemic may acquire peculiarity of character from an epidemic source, and how their cause may promote the extension of specific as well as epidemic fever. The same kind of synthesis may be observed in many other forms of disease which have given rise to much controversy; thus the pestilential fever of the *Eclaire* appears to have proceeded from the exhalations from the foul mud that covered the bottom of the ship, combined with African remittent, and the contagious form of yellow fever in the West Indies to be the result of local impurities, combined with the tropical remittent of that climate. Of such combinations, the epidemic of the present season presents one of the most difficult and complicated examples. Both dysentery and cholera are at present epidemic in various parts of the country, and dysentery is a most powerful predisponent to the operation of the cause of cholera, but more especially so when it combines with the putrefactive causes to

prostrate the vital powers. We shall have occasion to show in the sequel that a most contagious form of dysentery may arise from the concentrated exhalations of putrid exuviae, and where the epidemic cause also exists the combination of the two becomes virulent. This compound disease at present prevails among our cellar population, and appears to be allied to that which broke out among the children at Tooting, epidemic cholera being superadded to contagious dysentery. Such cases very rapidly fall into a state of almost hopeless collapse; and although this very active disease is not very readily conveyed to those who have not been subjected to the same putrid atmosphere, and its extension is easily checked by change of air, &c., it is impossible to doubt its communicability by contagion, or that the conveyance of the putrid poison of dysentery has in many cases induced intestinal derangement that has terminated in cholera.*

* There is a wide distinction between the secondary cholera above alluded to and the pure unmixed disease; for dysenteric cholera commences while the tongue and extremities are warm, with purging and vomiting of thin mucous fluid that is evidently a *secretion*, such as that produced by jalap or croton, and urine is passed with each evacuation, whereas in primary cholera the tongue is cold, the nails are blue, and secretion of urine has ceased previously to the first purging, and the stools are serous exudations.

Primary cholera begins in the peripheral circulation by loss of those vital properties of the capillary vessels by which the blood is preserved fluid and unadherent to their tubes, and by which those reactions between the blood and tissues are wrought, of which animal heat is one effect. The veins of the extremities contract, forcing their blood inwards upon the heart, and then its transmission through the lungs being impeded by a similar condition of the pulmonary capillaries, and retained by the valves of the external veins, it regurgitates upon the intestines and filters its fluid through their mucous membranes, so that great mechanical distension and congestion of the cavæ must here precede the purging. Our efforts must therefore be directed to restore and promote vital action in the paralysed capillaries, and to relieve congestion.

In secondary cholera, on the other hand, this condition does not precede the purging, but follows as a consequence of vital resistance to the epidemic influence of cholera having been subdued by the previous active purging; but cholera is not a necessary consequence of such attacks of dysentery, for in several districts in and around Manchester at this moment, very severe dysentery prevails, without being succeeded by collapse, (or very rarely,) while in another district dysenteric symptoms are liable to be immediately followed by that condition, unless prevented by most energetic treatment, of which the object is not to fortify but to subdue vital irritability in the intestinal vessels by emetics, calomel, and opium, lead, freezing mixtures, &c. When in such cases collapse follows, we see it commencing in the extremities, but instead of congestion following the rejection of their blood into the

We are convinced that the more we extend our observations of the influence of the putrefactive causes in rendering the system susceptible to the other causes of disease, the more we shall be satisfied that legislative enactments to enforce the removal of them are truly entitled to be styled laws "for the prevention of diseases;" while, therefore, we have been constrained by a due regard to correct medical theory, to resist the doctrines upon which the Board of Health has unfortunately based its arguments against quarantine, we can with pleasure concur with their excellent practical conclusions, that every improvement in drainage, ventilation, cleanliness, temperance, and the moral as well as social condition of the people, is a step towards the prevention of disease, and we cannot doubt that if the present war against filth proceed, as we have reason to hope that it will, now that its evils have been so ably exposed, England will never more have occasion to dread a recurrence of those horrors of putrid pestilence that were witnessed in the middle ages, when sweating sickness, black death, and plague desolated the land, or such melancholy destruction of the population as filth and famine, combined with specific and epidemic disease, have lately been working in Ireland.

We cannot certainly entertain the hope that all the causes of endemic disease can be rooted out and fever eradicated, so long as organic substances are liable to decomposition, and poverty is an evil, but this is no reason why we should cease from exertion till all known and acknowledged sources of putridity and pestilence, as cess-pools and burial grounds, are removed from our doors. But while we are anxious to give to the authors of these Reports the full meed of credit due to their exertions, we must not be restrained from pointing out the evil that must result if the theoretical doctrines they have propounded be generally received and acted

cavæ, and causing the peculiar struggling action of the heart observed in the primary disease, its action is feeble and subdued, because the vessels previously so excited appear at once to abandon all resistance to their distension, and the blood is no sooner driven from the extremities than its fluid is decanted off into the bowels, passive exudation of serous fluid almost instantaneously succeeding to active secretion of mucous dejections.

In the primary disease, so soon as congestion is relieved and capillary circulation is restored, the purging ceases; but in the secondary disease, if the patient recover from the collapse, purging still continues, because active dysenteric secretion returns and runs its course.

upon. To teach "that no contagion can cause a disease to spread without the presence of an epidemic atmosphere," is, in other words, to declare that specific fevers, such as scarlatina, typhus, small-pox, measles, &c., are not contagious, and that the separation of the sick from the healthy is of no avail in preventing their diffusion. Thus, instead of encouraging the adoption of the only means by which the poor have it in their power to preserve their families and neighbourhood from infection, the direct tendency of the Report on Quarantine is to dissuade them from sending those labouring under such diseases to the fever hospital. The consequence of this mistaken theory, therefore, is, that at the very moment that they conceive they are adopting means which will result in the total eradication of every species of fever from the country by removal of the *predisposing* causes, the commissioners are advising, with the whole weight of their authority, neglect of the *immediate* cause of specific fever—direct contagion,—and are thereby ensuring the perpetuation of this particular class of diseases.

Having now considered the principal causes of those diseases which are liable to spread generally among communities, both separately and in their combinations, I ought, perhaps, to conclude; but as it will be obvious that several very important maladies, all of which are highly contagious, such as hospital gangrene, phagedena, phlebitis, traumatic erysipelas, puerperal fever, gonorrhœa, ophthalmia, &c., have not been included, I am tempted to trespass a little longer on your patience, in order to embrace a part of the subject, without which, even as a sketch, the object proposed would remain incomplete. I shall not, however, abuse your indulgence by attempting more than to indicate my own impression, that the principles already partially discussed, of the operation of specific and general exciters on organic masses composed of various materials, may be applied to these diseases also, so as to promise both interest and instruction from the investigation.

The diseases just enumerated have been very variously classified by nosologists; but viewed in reference to their causes, it would appear that they may all be arranged under one general head, as effects of chemical decomposition acting on parts external to the blood, that is, upon the tissues and secretions formed from

the blood, rather than on the mass of the blood itself, or on materials contained in it. Adopting Liebig's principle, that matter in a state of active transformation from an organic to an inorganic condition, can operate as an exciter to chemical action in substances of identical chemical composition, and on them alone, even though they be mixed up with a variety of other different materials. We applied this law to the explanation of specific constitutional disease by considering the action of special exciters on the mixed materials of the blood. We also alluded to those more general exciters to decomposition, which are integral portions of a very compound mass, and capable of reducing the whole of the materials of that mass to their own condition of general putrefaction; but these we also considered chiefly in reference to their operation on the blood, and as the ordinary sources of putrid or endemic fever. We have now, however, to observe the action of similar exciters, not on the blood, but on the tissues and secretions, that have been formed from it; and in most of the instances to which this principle is now to be applied, we shall find very great variety in the condition of the exciters, and in the character of decomposition induced, according to circumstances, and to the nature of the material to which they are applied, and from which they arise. We have regarded the *specific* exciters of constitutional disease as acting only on a single element of a compound mass; but in reference to the putrefaction of external parts, we shall find them varying almost infinitely from those produced from, and capable of acting on, a single constituent of the tissues, to those involving more than one material, until they include the whole mass of the compound matters; we shall observe different conditions of decomposition induced upon similar materials by extraneous circumstances, and exciters emanating from them that are capable of reproducing the same condition. To take a familiar example,—if a cheese be divided into several portions, and each be placed under circumstances calculated differently to affect its decomposition, they will soon be found in very different conditions of decay. That which is kept moist and warm will have become absolutely putrid, while another, better preserved, will be blue-moulded, and a third, kept dry, will gradually crumble into dust. The connoisseur in cheese is very curious in producing exactly

the species of decay which best suits his palate by attention to such circumstances; but when he has thus once obtained a morsel exactly to his liking, he knows that it is not necessary to go through the same tedious and uncertain process to make another cheese succeed his favourite with the same flavour. He has only to choose one of similar character, and introduce portions of the old into incisions made in the new, and he may await with confidence the result of this practical propagation of a semi-specific *eremacausis*. So it appears to be with the different kinds of putrefaction capable of being induced in external wounds in the living body.

We have already alluded to one mode by which putrefaction may be induced on a wound, in the instance of the cook's hand or dissecting-room finger, by direct application of the exciter; but the same may also be effected indirectly by the absorption of putrid particles by the lungs: thus if a person on the verge of putrid typhoid fever receive a wound, it will be liable to take on putrefactive action. Bruised portions, of which the vital functions have been impaired, will acquire the tendency to putrescence from the active particles contained in the blood itself, and aided by exposure to the air, the edges of the wound will slough, and the putrefactive action be readily propagated along parts of low vitality, such as the sub-cutaneous tissues of the scalp. Erysipelas, with low irritative fever, will then be the consequence, just as we have seen ensue from putrefactive action propagated up the arm from wounds received in dissection. The putrid exhalations from this erysipelas being diffused in the atmosphere, and coming in contact with a wound in a more healthy person, propagate their own action there by direct application; thence it is again exhaled, till at length the whole air of a surgical ward is so contaminated that every wound or abrasion takes on the same character of disease. But it is not always the same character of putrescence that is thus engendered; local causes appear to exercise as much influence in its modification as we have seen to be the case in the decomposition of cheese; and just as one cupboard or cellar will make cheese ripen, while another, to all appearances similar, will make it rot, so will one ward often be found to give one character to stumps after amputation, and another a different one. In one, perhaps, they heal

kindly at first, but begin to ulcerate after the first dressing; in another, they become gangrenous; in another, without becoming sloughy, they are flabby and indisposed to heal—are watery and ichorous, as if the cellular tissue and the serous secretions were most in fault, a condition which appears peculiarly liable to extend itself to the divided veins. We have then phlebitis; and after one such case occurs in a ward, inflammation of every wounded vein appears to become endemic in that ward till whitewashing and other disinfecting processes are had recourse to.

The appearance of ulcers in hospital is an excellent indication of the healthiness of the wards, for after over-crowding or neglect of periodical purification, a change of weather will often be accompanied by the assumption of one general character in every abraded surface, or commencing in one case, it will soon spread to the others, whatever the origin of the sore or difference in the constitutions of the patients; and if they become foul, sloughy, phagedenic, or erysipelatous, they do so nearly alike. In some badly situated hospitals, such as the Hotel Dieu, which lies low upon the Seine, hospital gangrene is so liable to break out, that formerly, for whole seasons, every portion of the tissues exposed by wound or removal of the skin, took on putrefactive action, now dry and inodorous, now humid and fetid, but equally communicable by emanations, and every patient operated on died. There can, therefore, be no doubt that external circumstances may exercise as much influence in giving peculiarity of character to putrefactive action on living tissues exposed to their operation as we have found to be the case in cheese; and there is as little doubt that the species of decomposition so generated may be propagated by active particles from parts in such conditions, even beyond the influence of the circumstances by which they were originally produced, just as peculiarity of character is given to the decay of cheese by inoculation with a portion in that state brought from a distance. Some of these molecular exciters are more permanent than others, and some are more active, buoyant, and diffusible in the atmosphere, and more or less capable of attaching themselves to furniture, walls, floors, &c., and of there remaining dormant so long as they are dried up, but ready to resume their activity under the influence of moisture. For example,—some years ago the Manchester Infirmary was seldom

free from erysipelas, but by dry rubbing, instead of washing the floors, this disposition was greatly overcome; but this was an innovation strenuously opposed by the matron, and whenever an opportunity offered, she seized upon it to have the floors of the wards washed, the invariable consequence of which was a fresh outbreak of erysipelas where none previously existed. Similar facts are on record of the repeated outbreak of contagious ophthalmia and dysentery, where it was believed that the tendency had been overcome, and of these diseases being conveyed into barracks by a regiment, and the regiment which succeeded in their tour of occupancy becoming infected, though the contagion had evidently not been indigenous there but imported. We have had both ophthalmia and dysentery introduced into our wards by the inhabitants of cellars, and seen it spread rapidly where it did not exist previous to the admission of such patients; but these are contagious diseases, of a more specific character than those which have previously occupied our attention, in which the general materials of the frame were affected by gangrene or ulceration, and require to be distinguished from them as well as from specific fever or constitutional disease, for they operate on an external secretion, not on matters circulating in the blood, nor even directly on the actual structures of the body.

It would appear that the cause of contagious ophthalmia is a particular rearrangement of the constituent elements of the mucus of the eye, produced by the contact of particles emanating from the same secretion while undergoing a special kind of decomposition. The effect of its application is change of the bland lubricating fluid externally, similar to that which takes place internally in the *materies* of small-pox in the blood,—viz., its conversion to an irritating poison, which causes inflammation of the conjunctiva, so that it is not, in fact, a specific inflammation, but inflammation produced by the presence of a specific irritant. But this exciter can act on no mucus in the body of different composition; it does not convert that of the nares, or fauces, or digestive organs, with which it necessarily comes in contact, neither does it affect the elements of the mucus of the eye as they exist in the blood, nor till they are actually secreted as mucus, otherwise it would become a constitutional disease, probably an exanthematous fever. The irritation produces inflammation of the secreting membrane,

and thereby changes the secretion into pus, and so puts a termination to the disease by stopping the supply of that particular secretion, on which alone the exciter can operate. We cure this disease, artificially, by the frequent application of powerful antiseptics. We know, too, by experience that the exciter to this particular state of decomposition in the lubricating fluid of the eye is very subtle, and capable of producing its effects on its own *materies* in the eyes of others, even after having been wafted by the air to great distances, and after it has lain long inactive.

In gonorrhœa the cause is similar, but the specific transmutation effected by the exciter appears to be confined to the secretions from the genito-urinary mucous membrane, and not to be capable of extending its action into that of the bladder. Here, also, the natural cure is effected by change in the nature of the secretion produced by the inflammation induced by the presence of the irritant; and here, also, the artificial cure is effected by the use of antiseptic applications.*

The only mucus in the body upon which the gonorrhœal exciter is capable of operating, except that of the urethra, is the secretion of the eye, causing gonorrhœal ophthalmia by its contact. We have remarked upon the inability of this exciter to produce its specific effect on the blood from which its mucus is formed, yet there is some reason to believe that the integral particles of its exciter may circulate in the blood without losing their activity, and produce their specific effect when evolved along with the particular secretions which it is capable of affecting; for it would appear that gonorrhœal matter introduced into the rectum is capable of inducing urethral gonorrhœa as well as gonorrhœal ophthalmia without direct contact.

The possibility of active putrescent particles being circulated along with the blood and expelled with the secretions unchanged, and still capable of acting as exciters, was formerly alluded to, and affords the most probable explanation of the effects produced in

* May not the above suggest the impropriety of the early use of emollient or very weak injections, as liable to convey the specific cause of the inflammation to the posterior portion of the canal?—and does it not argue in favour of the earliest possible application of antiseptic injections as powerful as can be borne, but confined only to the anterior portion of the urethra?—at the same time adopting the necessary remedies for local inflammation, whether induced by the specific irritant or by the injection.

other diseases by contagious particles which appear to have no means of entering the system but by respiration, and yet, without affecting the mass of the blood, operate on parts or secretions to which their application cannot otherwise be traced, as in puerperal fever and contagious dysentery. There is reason to believe that the concentrated exhalations from putrid exuviae not unfrequently become a local cause of dysentery, without the aid of epidemic influence; at least cases of very acute dysentery, of highly contagious character, are frequently met with in cellars with open soughs or contiguous to foul necessaries. This may, perhaps, be explained by the exhalations from putrid intestinal mucus, when absorbed into the blood and expelled into the colon in a state of activity, inducing their own action in the mucus contained in the intestinal canal; for it would be difficult to explain how a patient, taken from such a locality and placed in the ward of an hospital, should infect those in the next beds, on any other supposition than that the putrescent particles evolved from that patient, or the dejections, were absorbed with the breath and excreted into the bowels, and there converted the natural lubricating mucus of the colon into an irritant capable of inducing ulceration of the membrane. It may appear a more simple explanation to suppose the active particles swallowed with the saliva; but the extraordinary power possessed by the stomach to destroy putrefactive action, and the fact that it is the colon and not the small intestines or stomach that is generally affected in this form of disease, somewhat militate against that opinion.

If it be allowable to continue such speculations, they may suggest an explanation of the almost incredible, yet too true, accounts we meet with of the operation of putrefactive excitors upon the thousand lacerated uterine veins of lying-in women, for it is recorded that not only direct contamination from the hand of the operating midwife previously engaged with a case of puerperal fever or other contagious putrid disease, but even the presence in the room of persons who have recently visited those suffering from gangrene, crysipelas, phlebitis, typhoid fever, &c., or who have attended an anatomical demonstration without change of dress, has communicated this fatal disease to their patient without manual contact.

If we attempt to trace the operation of this putrefactive cause,

whether directly applied or reaching the susceptible part only through the circulation, we can scarcely avoid perceiving in the inflamed veins an analogy with gonorrhœa. Their inner coats are lubricated by a secretion peculiar to themselves; this, when exposed to the air, is apparently so susceptible of chemical alteration that almost any active putrescent particle will excite its decomposition and conversion, this spreads along the tubes from their ruptured orifices, the same irritating effects are produced as we perceive in the urethra, viz., inflammation and alteration of the secretion into pus, or plastic lymph and adhesion of the coats; the pus so formed circulates with the blood, and, obeying the law previously alluded to, is thrown out of the circulation like other noxious materials, and forms deposits in the lungs or joints, or destroys life.

This is certainly not the view usually entertained of the progress of inflammation along the veins, which is held to depend upon what Mr. Hunter has denominated continuous inflammation; and I may be too bold in suggesting the possibility that the progress of inflammatory action along serous and mucous membranes, in certain forms of bronchitis, peritonitis, &c., may sometimes depend more upon the condition of their secretions than originally on any peculiar activity of their vessels; yet the violent effects upon interior organs, produced by puncture of the hand, received in examining bodies very recently dead of acute diseases, appear to give some countenance to such speculations, though I am sensible that they are too unsubstantiable to be further insisted upon at present.

I have now only to apologise for the length to which this paper has extended, and especially to request your indulgence for such immature remarks as a desire to embrace the whole subject has led me into. I am not so vain as to believe that I have contributed novelty to the subject, nor sanguine enough to suppose that all I have advanced will meet your approval. My object will have been fully attained, if my attempts at definition of the several causes that conduce to the production and the variety of pestilence, fever, and other diseases, succeed in giving rise to reflection on a subject which, at the present moment, it is particularly desirable that the medical profession should well consider, and unite in their conclusions. If it shall appear that the sub-division of the causes of

general disease be actually such as they have been here considered, the hope may be indulged, that the ultra-contagionists and anti-contagionists may find, in the combination of epidemic with putrid fever, a common ground on which to adjust their differences, for it cannot but be considered highly detrimental to the respect due to the profession, that opinions apparently wholly irreconcilable should be held by many of its members on a subject so important as contagion.

If, in the discussion of these questions, I have appeared to have occasionally overstepped the bounds of ascertained knowledge, and especially in the latter part of the paper to have ventured on subjects beyond my ability, the kindness with which you have listened to me convinces me of your approval of the intention at least, if not of the execution, of my task, and that you are disposed to view with leniency any errors I may have fallen into in endeavouring to apply to the elucidation of some of the mysteries of disease a great, though only recently discovered, law of nature, to which it would appear that every product of vitality must ultimately submit.

ERRATUM.

At page 35, fifth line from the bottom,—*for* “Such as our living gaols once enclosed,” *read* “Such as our gaols once enclosed,” &c.

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